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How Does Online Lending Influence Bankruptcy Filings?

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Abstract. By providing quick and easy access to credit, online lending platforms may help borrowers overcome financial setbacks and/or refinance high-interest debt, thereby decreasing bankruptcy filings. On the other hand, these platforms may cause borrowers to overextend themselves financially, leading to a "debt trap" and increasing bankruptcy filings. To investigate the impact of online lending on bankruptcy filings, we leverage variation in when state regulators granted approval for a major online lending platform—Lending Club—to issue peer-to-peer loans. Using a difference-in-differences approach, we find that state approval of Lending Club led to an increase in bankruptcy filings. A complementary instrumental variables analysis using loan-level data yields similar results. We find suggestive evidence that the ease of receiving a Lending Club loan causes some borrowers to overextend themselves financially, leading to bankruptcy. Our results suggest that recent initiatives from online lending platforms to control how borrowers use loans, such as Lending Club's "balance transfer loans" that send loan funds directly to creditors, can help these platforms provide safe and affordable credit. Our study adds to the literature that examines how online platforms influence society and the economy; it contributes to the literature that examines how financial products, services, and regulations influence bankruptcy filings; and it has policy implications for online lending design and regulation.

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Introduction

Online lending platforms match borrowers with investors willing to lend their capital. These platforms have the potential to expand access to credit to borrowers who are underserved by traditional credit sources, such as banks, as well as to provide better terms to all borrowers. Indeed, online lending platforms market their ability to help borrowers improve their financial health.[1] We study the effect of online lending on financial health, as measured by bankruptcy filings. On one hand, the optimistic view (i.e., that online lending decreases bankruptcy) is that the increased access to credit that online lending provides will help borrowers handle unanticipated financial setbacks and stave off bankruptcy. For some borrowers, including those who are traditionally underserved, an online loan may help them remain financially solvent during times of financial need. The relatively low interest rate of online loans (compared with credit cards) may also help borrowers refinance existing debt, thereby reducing their debt burden and helping them avoid bankruptcy. On the other hand, the pessimistic view (i.e., that online lending increases bankruptcy) is that online lending will cause borrowers to take on more debt than they can service, driving them into bankruptcy. This could occur if online loans are issued to unqualified borrowers who cannot repay the loans. It could also occur if the convenience of obtaining an online loan causes otherwise qualified borrowers to overextend themselves financially, leading them into a "debt trap" and subsequent bankruptcy. Overall, the effect of online lending on bankruptcy filings is theoretically ambiguous and warrants empirical examination. Understanding the effects of online lending on social and financial outcomes is important for platform designers, policy makers, and regulators. For example, the U.S. Department of Treasury issued a white paper that characterized the industry as "untested" and called for ongoing monitoring of online lending and its outcomes.[2] This is particularly important, given that the online lending market is growing rapidly and has originated more than $80 billion in personal loans from 2006 to 2020.[3]

We focus our analysis on Lending Club, which is the largest online lending platform and is representative of other online lending platforms, such as Prosper and Funding Circle. To examine Lending Club's impact on bankruptcy filings, we conduct a difference-in-differences analysis in which we exploit variation in when state regulators granted approval for Lending Club to issue peer-to-peer loans. We find that Lending Club approval leads to an increase in bankruptcy filings. We also use microlevel loan data published by Lending Club to examine the relationship between lending activity and bankruptcy filings. We use an instrumental variables approach to improve the causal interpretation of our results. We find that a one-standard-deviation increase in Lending Club loans is associated with an approximately 3% increase in bankruptcy filings. We find suggestive evidence that the increase in bankruptcy filings is because some borrowers become overextended financially after receiving a Lending Club loan (as opposed to their being inherently uncreditworthy). Some of the increase in bankruptcy may also be due to strategic borrowing, in which borrowers use Lending Club loans to restructure their debt in a way that makes bankruptcy more attractive, thereby nudging them to file bankruptcy when they otherwise would not have. We also find suggestive evidence that traditional lenders respond to competition from Lending Club by issuing riskier loans, thereby contributing to the increase in bankruptcy.

Our study contributes to the online lending literature, as well as to the bankruptcy and household finance literature. First, as more online lending platforms/technologies are created, it is important to study their effects on access to capital, funding-allocation efficiency, and household financial stability (Mollick and Robb 2016, Butler et al. 2017, Wei and Lin 2017, Kim and Hann 2018, Burtch and Chan 2019). Our findings suggest that online lending platforms may have harmful effects and that design and/or regulatory changes may help these platforms provide safe and affordable credit. Indeed, Lending Club recently introduced "balance transfer loans," which send loan funds directly to the borrowers' creditor(s).[4] This type of program could address the effect that we document. Second, our study contributes to the bankruptcy and household-finance literature by investigating how online lending influences bankruptcy. This adds to existing studies that have examined how financial products/services, such as payday loans and credit cards, as well as regulatory changes, such as bankruptcy reform and interstate banking deregulation, influence bankruptcy filings (Dick and Lehnert 2010, Hynes 2012).

Background, Literature Review, and Motivation

We first discuss the literatures on bankruptcy and online lending. We then discuss how online lending platforms might affect bankruptcy.

Bankruptcy

Reasons for and Implications of Bankruptcy. Bankruptcy is a legal process used by individuals and businesses to resolve unpaid debts. Different theories have been proposed to explain why debtors file for bankruptcy, including strategic motive theory and adverse events theory. Strategic motive theory argues that debtors are motivated by financial benefits to file bankruptcy. These benefits include discharging (some) debt and stopping collection activities of creditors, including collection letters/phone calls/visits, wage garnishment, and other court orders (Lefgren and McIntyre 2009, Dawsey et al. 2013). The surge of bankruptcy filings prior to the Bankruptcy Abuse Prevention and Consumer Protection Act in 2005 (which reduced the benefits of filing for bankruptcy for many) provides support for the strategic motive perspective (White 2009). Adverse events theory considers bankruptcy to be a consequence of growing financial distress, potentially driven by unemployment, increasing housing and medical costs, divorce, credit card debt, and/or unfair and abusive practices by lenders (Dick and Lehnert 2010). Several studies support this theory by showing that unemployment is a major contributing factor to bankruptcy (Himmelstein et al. 2005, Zhu 2011). Whatever the reasons for bankruptcy filings, the downsides of bankruptcy are substantial and discourage filing, including harm to the debtor's credit score (that may take years to repair) and the cost of filing (both financially and in terms of social stigma).

Programs, Policies, etc. That Affect Bankruptcy. Given the negatives of filing for bankruptcy, researchers have studied financial products, regulations, and market activities that might affect bankruptcy. One stream examines the effect of payday loans, credit cards, and online platforms. The effect of payday loans is inconclusive. There is evidence that payday loans increase consumer bankruptcy filings and that Chapter 13 bankruptcies decrease after payday-loan bans (Morgan et al. 2012). However, the legalization of payday loans has been shown to reduce bankruptcy filings in counties with large military populations (Hynes 2012). Research on the effect of credit cards suggests that expanded credit-card debt contributed to an increase in bankruptcy from 1980 to 2004 (White 2007). This may be because the pool of consumers who were issued credit cards became riskier over time (Livshits et al. 2016). Research on the effect of online platforms is nascent (with our study being one of the first), although a recent study indicates that a medical crowdfunding platform (GiveForward) reduces bankruptcy by helping individuals cover unexpected medical costs (Burtch and Chan 2019). Another stream examines the effect of banking deregulation and bank mergers on bankruptcy. Research has shown that competition brought about by deregulation prompted banks to adopt sophisticated credit-rating technology, which they used to expand lending to previously excluded (and typically riskier) households. This explained at least 10% of the rise in bankruptcy rates between 1980 and 1994 (Dick and Lehnert 2010). Bank mergers have been shown to increase consumer bankruptcies because they destroy interpersonal, relational knowledge that lenders use to identify creditworthy borrowers and to shepherd them through financial difficulties (Allen et al. 2016).

Online Lending Platforms

Online lending platforms match borrowers with investors for personal or small-business loans. Online lending is also referred to as peer-to-peer lending, loan-based crowdfunding, and marketplace lending (Morse 2015). To illustrate how online lending works, we describe the typical model pioneered by Lending Club. First, a borrower requests a loan by providing personal information and the desired loan amount. Second, the online lending platform analyzes the borrower's information to assess risk and to assign an interest rate. Third, lenders/investors choose which loans to fund, using their own capital. They decide the amount they want to lend and can invest as little as $25 in each loan. If enough investors want to lend money to the borrower, then she can get the loan. There is a growing body of research about online lending, including how it affects access to capital, how the design and operation of online lending platforms affect lending outcomes, and how investors behave.

Access to Capital. Because online lending is an alternative funding source compared with banks, it has the potential to democratize access to capital (Mollick and Robb 2016). Indeed, research has concluded that online lending has penetrated areas likely to benefit from increased access to capital, including those with highly concentrated (i.e., weakly competitive) banking markets, those that are losing bank branches, and those of low socioeconomic status (Kim and Hann 2018, Jagtiani and Lemieux 2019). Online lending may also help borrowers with good access to traditional capital secure loans with attractive terms. For example, online lending borrowers from areas with good access to bank finance seek loans with low interest rates and (perhaps as a result) are more likely to prepay (Butler et al. 2017, Alyakoob et al. 2021).

Platform Design, Operation, and Outcomes. Research has studied how the design and operation of online lending platforms affects lending outcomes. For example, underwriting for online loans is based on more information and is faster than traditional underwriting (Buchak et al. 2018, Jagtiani and Lemieux 2019). This allows more borrowers to receive credit at favorable terms; Jagtiani and Lemieux (2019) show that online lending technology is more likely to classify a subprime borrower into a better loan grade compared with traditional lenders. It also allows loans to be issued more quickly. Other research has studied whether online lending platforms should assign interest rates to borrowers (i.e., the posted price regime) or allow investors to propose their own interest rates to borrowers (i.e., the auction regime). Wei and Lin (2017) found that the posted price regime yields more matches between borrowers and investors, but also yields higher default rates.

Investor Behavior. Online lending investors rely on both traditional financial information and "soft" information to make lending decisions (Iyer et al. 2016). In addition to traditional factors, such as credit scores, the decision process is influenced (and potentially biased) by several factors, including other investors' decisions, loan descriptions, borrowers' friendship networks, borrowers' demographics (including gender, race, and overall "appearance"), and the distance (geographical and cultural) between investor and borrower (Pope and Sydnor 2011, Galak et al. 2011, Zhang and Liu 2012, Duarte et al. 2012, Lin et al. 2013, Burtch et al. 2014, Harkness 2016, Lin and Viswanathan 2016, Younkin and Kuppuswamy 2018, Greenberg and Mollick 2017, Hildebrand et al. 2017, Hong et al. 2018). Sophisticated and less-sophisticated investors often rely on different information when determining whom to fund, but their investment returns are often similar (Mollick and Nanda 2016, Lin et al. 2020).

How Online Lending Might Affect Bankruptcy

There are multiple mechanisms through which online lending could affect bankruptcy filings (most of which we examine empirically in our analysis). These mechanisms relate to the characteristics of online lending borrowers and how they use online loans.

Characteristics of Online Lending Borrowers. It is possible that many online loans are issued to high-risk borrowers who lack access to traditional capital. If these borrowers are inherently uncreditworthy and unable to repay the loan, then online lending should increase bankruptcy filings. This might occur if investors' biases (such as those noted above) cause them to fund high-risk borrowers. On the other hand, online lending may provide the capital necessary for these borrowers to handle unanticipated financial setbacks and to remain solvent during times of financial need, which could decrease bankruptcy filings.

Use of Online Loans. Regardless of whether a borrower is high-risk or not, how borrowers use online loans should influence bankruptcy filings. If borrowers use the loans to refinance high-interest debt, then online lending would reduce their debt burden, leading to fewer bankruptcies. On the other hand, if borrowers use the loans to add to existing debt, then online lending would increase their debt burden, leading to more bankruptcies. To illustrate, consider the following example. Assume that person Z has an average credit-risk profile and has $13,000 in credit card debt at a 20% interest rate. (According to the 2007 Consumer Bankruptcy Project, median credit card debt was $13,279 for bankruptcy filers.) Assume that Z gets a $13,000 online loan with a 13% interest rate. If Z pays off his credit card debt with the online loan, then he will have $13,000 in debt at a 13% rate instead of at a 20% rate. This improves his financial situation, helping him avoid a potential bankruptcy. However, if Z does not pay off his credit card debt and instead uses the online loan for other purposes, then he will have $13,000 in credit card debt at a 20% rate plus $13,000 in online loan debt at a 13% rate. This worsens his financial situation, potentially driving him into bankruptcy.

A working paper (Chava et al. 2021) using credit-bureau data suggests a related possibility. It shows that many borrowers use online loans to pay off credit card debt, as intended. Because this increases borrowers' credit ratings, they receive—and often accept—additional credit offers. Ironically, this often leads to more aggregate credit card debt and subsequent default—and potentially to bankruptcy.

Another possibility is that online lending provides an incentive for borrowers to file for bankruptcy when they might not otherwise. Consider a borrower who owes $10,000 on a car loan that he is struggling to repay. He may be hesitant to file bankruptcy because this might result in losing his car. But suppose that he uses an online loan to pay off his car loan—thereby swapping secured debt for unsecured debt—and then files for bankruptcy.[5] This type of debt restructuring may be tempting, because it may protect a borrower's property (e.g., car) from repossession during bankruptcy.[6] If this type of "strategic" borrowing occurs, then online lending could increase bankruptcies by nudging marginal borrowers to file.

Overall, it is unclear a priori whether online lending has a positive or negative effect on bankruptcy filings, or which mechanisms are responsible for any effect. This motivates our empirical examination.

Empirical Setting and Analysis Strategy

The online lending platform we investigate is Lending Club. We chose this platform for three reasons: (1) It is the largest online lending platform; (2) it was approved to issue peer-to-peer loans in different states at different times, which provides a natural experiment that we leverage to examine its effect on bankruptcy filings; and (3) it publishes microlevel loan data. We leverage the staggered approval of Lending Club across states in a difference-in-differences ("DiD") analysis to examine its impact on bankruptcy. This strategy of exploiting staggered approval/entry has been implemented in several studies that investigate the impact of regulatory change and platform implementation (Bertrand et al. 2004, Chan and Ghose 2014, Greenwood and Agarwal 2016, Burtch et al. 2018). We also use Lending Club's loan data to examine the relationship between the level of Lending Club loans and bankruptcy filings, using instrumental variables to improve the causal interpretation of our results. Using both difference-in-differences and instrumental variables approaches allows us to leverage the benefits of each (e.g., Seiler et al. 2017, Li and Netessine 2019, and Atanasov and Black 2021).

Overview of Difference-in-Differences Approach

Lending Club launched its platform in 2007. In April 2008, Lending Club entered a "quiet" period, in which it suspended peer-to-peer lending until it registered with federal and state regulators as a licensed lender (or loan broker). During the quiet period, Lending Club funded some loans with its own money (instead of with investors' money), but these loans were few (see Online Appendix A for an illustration).[7] Lending Club pursued regulatory approval to resume peer-to-peer lending in all 50 states. By October 2008, it had received approval in 40 states, plus the District of Columbia. For nine states, it received approval at different times between 2010 and 2016. For one state (Iowa), it had not received approval as of February 2021.[8] Table 1 shows the quarter in which Lending Club received regulatory approval in each state. We gathered this information from Lending Club's blog, from news about Lending Club, and by using Lending Club's loan data to examine lending activity in each state over time. The variation in when states allowed Lending Club to resume peer-to-peer lending provides a natural experiment that we exploit to examine the impact of Lending Club on bankruptcy filings.

Because bankruptcy data are available for U.S. counties on a quarterly basis starting in 2008, we construct a county-quarter panel spanning 2008 to 2014. We conduct analysis at the county level rather than the state level. This improves our identification because Lending Club was approved at the state (not county) level. Thus, even if unobserved state-level factors influence both Lending Club approval and bankruptcy filings, these factors may not apply at the county level. Analysis at the county level also allows us to

Table 1. Lending Club Approval by State

State	Approval quarter	Approval quarter (first full quarter after approval, as coded for analysis)
All states, except those listed below	2008-Q4	2009-Q1
Kansas	2010-Q4	2011-Q1
North Carolina	2010-Q4	2011-Q1
Indiana	2012-Q4	2013-Q1
Tennessee	2013-Q1	2013-Q2
Mississippi	2014-Q2	2014-Q3
Nebraska	2015-Q2	2015-Q3
North Dakota	2015-Q2	2015-Q3
Maine	2015-Q3	2015-Q4
Idaho	2016-Q1	2016-Q2
Iowa	Not approved as of 2021-Q1	Not approved as of 2021-Q1

control for county-level demographic and economic variables, thereby improving the precision of our estimate of Lending Club's impact.

We estimate the effect of Lending Club approval using a difference-in-differences approach, with the counties in the states in which Lending Club hadn't yet been approved or was not approved during the analysis period serving as the counterfactuals for the counties in the states in which Lending Club was approved. We use two samples. The first sample includes counties from all 50 states plus the District of Columbia ("all-state analysis"). The second sample includes counties from the nine states that approved Lending Club no earlier than 2010 ("nine-state analysis"). The all-state analysis has the advantage of including counties from across the United States. However, a concern with the all-state analysis is that a majority of states approved Lending Club at the same time (2008-Q4). This approval time might coincide with an unobserved event(s) that confounds our findings. The nine-state analysis helps us address this issue and other shortcomings of the all-state analysis. The nine-state analysis consists of counties within four states (Kansas, North Carolina, Indiana, and Tennessee) that were "treated" with Lending Club approval between 2010-Q4 and 2013-Q1 and counties within five "control" states (Nebraska, North Dakota, Maine, Idaho, and Iowa) in which Lending Club was not approved by 2014.[9] The four treated states approved Lending Club in three different quarters (see Table 1). Thus, any confounding event would have had to occur in each of these states at the same time as Lending Club approval (and at no other time and not in the control states), which is unlikely. Furthermore, we observe each county in the nine-state analysis for at least 12 quarters before Lending Club approval. This helps us assess whether pre-existing trends in bankruptcy filings might confound the effect of Lending Club approval. (Our pretreatment observation window is shorter for the all-state analysis.) Last, Lending Club was relatively well-established and likely to be known by prospective borrowers by 2010, when the first states in the nine-state analysis were treated. This increases the likelihood that Lending Club approval will have a detectable effect.

All states (except for Iowa) approved Lending Club by at least 2016 (see Table 1). This suggests that there may be no dramatic difference between the control and treated states in terms of their overall attitude to online lending, only differences in how long it took Lending Club to receive the necessary regulatory approvals. This increases the likelihood that counties in the control states are valid counterfactuals for counties in the treated states.

Data, Analysis, and Results

Data and Variables

Bankruptcy Filings. The key dependent variable is *bankruptcy filings per capita*, which is the number of bankruptcy filings per 1,000 people in county i in quarter t. We obtained bankruptcy filing data from the Federal Judicial Center and the Administrative Office of the U.S. Courts websites.[10] In addition to *bankruptcy filings per capita*, we also use the raw number of bankruptcy filings and the natural log of the raw number (plus one to account for values of zero). These measures are widely used in bankruptcy studies (Dick and Lehnert 2010, Burtch and Chan 2019).

Lending Club Available. The key independent variable is *Lending Club available*. This variable is one if Lending Club is available to borrowers in county i in quarter t, and zero otherwise. We defined Lending Club as available in the first full quarter after Lending Club approval. For robustness, we used an alternative coding rule (see below), which does not affect our results.

Demographic and Socioeconomic Information. We include several demographic and socioeconomic control variables gathered from the U.S. Census and the U.S.

Bureau of Labor Statistics. These include population, number of employed individuals, average monthly earnings (per individual), size of the labor force, and median household income. This allows us to control for alternative explanations and improves the precision of our estimate of the effect of Lending Club. Each variable is available at the county level. The number of employed individuals and average monthly earnings are available quarterly. For variables available only yearly, we use the yearly value to proxy for quarterly values in the analysis. We also collected data about county residents such as percentage age 60 and above, percentage white, and percentage with below high school educational attainment, as well as percentage of housing units with mortgages. Because these variables are not always reported for small counties, we include them only in robustness checks. The results are similar when we drop observations with missing control variables or when we impute the values for missing control variables. In the interest of transparency and so that others can replicate our results, we provide the data and regression commands for most of our analyses. See the readme.pdf file in the data and code submission replication materials online for details.

Empirical Strategy for Difference-in-Differences Analysis

Our baseline difference-in-differences specification is shown below.

$$Y_{it} = \alpha + \beta LC_{it} + T_t + C_i + \gamma X_{it} + \varepsilon_{it}. \quad (1)$$

Y_{it} is the number of bankruptcy filings per capita in county i in quarter t. LC_{it} is a dummy variable equal to one if Lending Club is available to borrowers in county i during quarter t, and zero otherwise. α is a constant term, T_t are quarter fixed effects, C_i are county fixed effects, X_{it} are control variables, γ are associated coefficients, and ε_{it} is the error term, which is clustered at the county level (and alternatively at the state level, which does not affect our results). The quarter fixed effects account for changes over time that affected bankruptcy filings, which were substantial during the study time period because of the Great Recession. The county fixed effects account for unobserved time-invariant characteristics of each county. The control variables help us better estimate the effect of Lending Club. The parameter of interest is β, which represents the average treatment effect of Lending Club approval on bankruptcy filings.

Ideally, our sample would include treated and control counties that had parallel bankruptcy trends prior to Lending Club approval in the treated counties. This would increase the likelihood that a change in bankruptcy filings in the treated counties following Lending Club approval was caused by Lending Club approval. However, pretreatment bankruptcy trends are not parallel in either the all-state analysis or the nine-state analysis. To account for this, we used coarsened exact matching (CEM) to build a matched sample for both analyses, in which the pretreatment trends for treated and control counties are parallel. For the all-state analysis, we matched treated and control counties based on the quarterly values of *bankruptcy filings per capita* and *number of employed individuals* from 2008-Q1 to 2008-Q4—that is, prior to Lending Club approval. We coarsened these variables into equally spaced bins and only matched treated and control counties within the same bins. We also matched on *population* (in 2008-Q4). The matching yielded 74 matched strata that each contained at least one treated and one control county. These strata contained 2,309 counties in total, which comprise the matched sample: 2,020 treated counties matched to 289 control counties. For the nine-state analysis, we matched treated and control counties based on quarterly values of *bankruptcy filings per capita* and *number of employed individuals* from 2008-Q1 to 2010-Q4—that is, prior to Lending Club approval.[11] We also matched on *population* (in 2010-Q4). The matching yielded 57 matched strata that each contained at least one treated and one control county. These strata contained 339 counties in total, which comprise the matched sample: 155 treated counties matched to 184 control counties. A characteristic of matching procedures, reflected in our study, is that a treated observation is sometimes matched to more than one control observation, and vice versa. To accommodate this, the CEM algorithm generates a weight for each county, which we use in our analysis (Iacus et al. 2012). For robustness, we also matched each treated county to a single control county, which affects our sample size, but not our results. We checked the balance between treated and control counties in both the all-state matched sample and the nine-state matched sample by running several regressions of the form $Y_i = \alpha + \beta Treated_i + \varepsilon_i$. Y_i is one of the matching variables (e.g., *bankruptcy filings per capita* in 2008-Q4, *bankruptcy filings per capita* in 2009-Q4, etc.) and $Treated_i = 1$ for treated counties and zero for control counties. We included the counties in the matched samples and used the weights generated by the CEM procedure. Online Appendix B shows that we achieved balance on not only the matching variables, but also on variables not included in the matching procedure. Tables 2 and 3 show the descriptive statistics of the two matched samples.

Leads/Lags Model. To examine whether the treated and control counties have parallel bankruptcy trends prior to Lending Club approval (i.e., pretreatment),

Table 2. Variables and Descriptive Statistics of the All-State Matched Sample

Variable	Source	Min	Max	Mean	Median	St. Dev.
Bankruptcy variables						
Bankruptcy filings per capita	Federal Judicial Center	0	6.92	0.66	0.61	0.42
Bankruptcy filings - raw	Federal Judicial Center	0	1194	46	16	84
Bankruptcy filings - natural log	Federal Judicial Center	0	7.09	2.82	2.83	1.50
Online lending variables						
Lending Club available (binary variable)	Lending Club data, news reports	0	1	0.69	1	0.46
Prosper.com available (binary variable)	Prosper.com data, news reports	0	1	0.70	1	0.46
Control variables (used in main analysis)						
Population (in thousands)	Population Estimates Program	0.26	543.99	58.45	25.54	86.25
Number of employed individuals (in thousands)	Quarterly Workforce Indicators	0.06	296.77	19.75	7.10	34.87
Average monthly earnings (in thousands)	Quarterly Workforce Indicators	1.32	23.76	3.02	2.91	0.67
Median household income (in thousands)	Small Area Income and Poverty Estimates	18.86	125.64	44.81	43.05	11.12
Labor force (in thousands)	Local Area Unemployment Statistics	0.18	306.15	28.92	11.89	44.15

Notes. Means and standard deviations (St. Dev.) are calculated using the CEM weights. Because *number of employed individuals* and *labor force* come from different surveys and are measured via different approaches, we include both instead of calculating a single *unemployment rate* variable. This approach controls for unemployment rate without introducing potential errors associated with calculating unemployment rate.

we implemented a leads/lags model, shown in Specification (2) (Autor 2003):

$$Y_{it} = \alpha + \sum_{\tau=-8}^{-2}\rho_\tau LC_{it+\tau} + \sum_{\tau=0}^{8}\rho_\tau LC_{it+\tau} + T_t + C_i + \gamma X_{it} + \varepsilon_{it}. \tag{2}$$

Specification (2) mirrors (1) except that we replace βLC_{it} with $\sum_{\tau=-8}^{-2}\rho_\tau LC_{it+\tau} + \sum_{\tau=0}^{8}\rho_\tau LC_{it+\tau}$. $LC_{it+\tau}$ is a dummy variable equal to one for observations for county i in quarter t if quarter t is τ quarters after Lending Club approval (or for $\tau < 0$, $-\tau$ quarters before Lending Club approval). We withhold LC_{it-1} to avoid the "dummy variable trap." For example, we coded Lending Club as being approved in North Carolina in 2011-Q1. Thus, for counties i in North Carolina, $LC_{it-1} = 1$ for the 2010-Q4 observations and zero otherwise; $LC_{it+0} = 1$ for the 2011-Q1 observations and zero otherwise; $LC_{it+1} = 1$ for the 2011-Q2 observations and zero otherwise, etc. For counties in which we observe more than eight pretreatment and/or posttreatment quarters, we collapse the preceding and/or following quarters into the −8 and/or +8 time periods.

Recall that we use a county-quarter panel spanning 2008 to 2014. Some control counties in our analysis were treated after 2014, such as those in Nebraska, which approved Lending Club in 2015-Q2 (ergo, we coded the approval quarter as 2015-Q3; Table 1). Thus, for counties i in Nebraska, $LC_{it-3} = 1$ for the 2014-Q4 observations and zero otherwise; $LC_{it-4} = 1$ for the 2014-Q3 observations and zero otherwise, etc. Because Lending Club had not been approved in Iowa as of February 2021, we set all $LC_{it+\tau}$ variables for observations from Iowa counties to zero.[12]

Results of Difference-in-Differences Analysis

Table 4 shows the results of our baseline model (Specification (1)) for the all-state matched sample and the nine-state matched sample with both *bankruptcy filings*

Table 3. Variables and Descriptive Statistics of the Nine-State Matched Sample

Variable	Min	Max	Mean	Median	St. Dev.
Bankruptcy variables					
Bankruptcy filings per capita	0	5.28	0.54	0.50	0.36
Bankruptcy filings - raw	0	243	17	8	24
Bankruptcy filings - natural log	0	5.50	2.15	2.20	1.25
Online lending variables					
Lending Club available (binary variable)	0	1	0.24	0	0.43
Prosper.com available (binary variable)	0	1	0.48	0	0.50
Control variables (used in main analysis)					
Population (in thousands)	0.65	295.32	27.76	15.36	34.79
Number of employed individuals (in thousands)	0.10	175.63	9.67	4.62	16.22
Average monthly earnings (in thousands)	1.48	7.85	2.80	2.73	0.56
Median household income (in thousands)	25.25	86.35	44.42	44.17	7.64
Labor force (in thousands)	0.43	161.38	13.98	7.70	17.99

Notes. Means and standard deviations (St. Dev.) are calculated using the CEM weights. Data sources are the same as those listed in Table 2.

Table 4. Regression Results for Bankruptcy Filings (Per Capita and Natural Log)

Sample	All-state matched sample		Nine-state matched sample	
Dependent variable	Bankruptcy filings per capita	Bankruptcy filings - natural log	Bankruptcy filings per capita	Bankruptcy filings - natural log
Lending Club available	0.034 (0.008)***	0.050 (0.010)***	0.050 (0.017)***	0.089 (0.024)***
Population	−0.003 (0.001)***	0.002 (0.001)**	−0.003 (0.004)	0.001 (0.007)
Number of employed individuals	−0.008 (0.001)***	−0.006 (0.001)***	−0.004 (0.005)	−0.010 (0.009)
Average monthly earnings	0.019 (0.006)***	0.002 (0.008)	−0.012 (0.022)	−0.017 (0.032)
Median household income	−0.001 (0.001)	−0.004 (0.001)***	0.002 (0.002)	0.004 (0.003)
Labor force	0.004 (0.001)***	0.000 (0.002)	0.010 (0.008)	0.004 (0.012)
County fixed effects	✓	✓	✓	✓
Quarter fixed effects	✓	✓	✓	✓
n (counties)	2,309	2,309	339	339
n (observations)	64,590	64,590	9,492	9,492
R^2, including fixed effects	0.675	0.951	0.488	0.908

Note. Regressions are weighted using the CEM weights and standard errors (in parentheses) are clustered by county.
$^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

per capita and *bankruptcy filings - natural log* as the dependent variables. Lending Club approval has a positive and significant impact on bankruptcy filings. The per capita model indicates that Lending Club approval increases bankruptcy filings by 0.034 per thousand people in the all-state matched sample and 0.050 per thousand people in the nine-state matched sample. This represents average increases of 5.2% (i.e., 0.034/0.66) and 9.3% (i.e., 0.050/0.54), which are similar to the increases shown in columns (2) and (4) for the logged models. The smaller effect size in the all-state analysis may be because this analysis contains treated observations from years in which Lending Club was very new (e.g., 2009 and 2010). Online Appendix C reports the results of a Poisson model and a negative binominal model using *bankruptcy filings – raw* as the dependent variable. Results are similar. The unreported time fixed effects follow intuition: Bankruptcy filings increased until 2009-Q3 and then decreased, likely due to the Great Recession.

Table 5 shows the results from the leads/lags model (Specification (2)) for *bankruptcy filings per capita*. In both the all-state and nine-state matched samples, the *Lending Club(-8)* (i.e., LC_{it-8}) to *Lending Club(−2)* coefficients are insignificant (recall that the omitted "baseline" dummy variable is *Lending Club(−1)*). The *Lending Club(+0) to Lending Club(+8)* coefficients show that the effect of Lending Club on bankruptcy filings grows larger and more statistically significant over time, particularly when the quarterly time periods are grouped by year to smooth out seasonality. In both analyses, the *Lending Club(+8)* coefficient is significantly larger than the *Lending Club(+0)* coefficient. This may be because usage of Lending Club is relatively low in the approval quarter. Overall, the results indicate that the effect of Lending Club only becomes apparent in treated counties after Lending Club is approved, which is consistent with the assumption of parallel pretreatment trends. Online Appendix D plots the lead and lag coefficients with their 95% confidence intervals. In unreported analysis, we included dummy variables for *LendingClub(+9)* through *LendingClub(+24)* in the all-state analysis (and *LendingClub(+9)* through *LendingClub(+15)* in the 9-state analysis). These coefficients are similar to that for *LendingClub(+8)* reported in Table 5, which suggests that the effect stabilizes. We also used alternative numbers of leads/lags, specifically −5 to +5 and −12 to +12. Results are similar to those reported in Table 5.

We conducted several robustness checks, including: (1) using an alternative coding rule in which we considered Lending Club to be available immediately upon approval; (2) clustering the standard errors by state; (3) including additional control variables (i.e., *% age 60 & above*, *% white*, *% below high school attainment*, and *% housing units with mortgage*); (4) winsorizing all variables at the 1% and 99% (and 5% and 95%) thresholds (see Online Appendix E); and (5) adding state-specific time trends (see Online Appendix F). Results remain robust. We also ran a placebo test, in which we randomly assigned *Lending Club available* within the all-state and nine-state panels. This placebo assignment yielded no significant effect.

Falsification Test: Nonbusiness vs. Business Bankruptcy Filings

To enhance the causal interpretation of our findings, we conducted a falsification test based on nonbusiness versus business bankruptcy filings, which the bankruptcy-filing data distinguish between. Given that the

Table 5. Regression Results for Bankruptcy Filings Per Capita: Leads/Lags Model

Sample	All-state matched sample	Nine-state matched sample
Dependent variable	Bankruptcy filings per capita	
Lending Club(−8)	0.008 (0.013)	0.037 (0.032)
Lending Club(−7)	0.002 (0.018)	0.017 (0.037)
Lending Club(−6)	0.013 (0.021)	0.053 (0.040)
Lending Club(−5)	0.005 (0.018)	0.010 (0.032)
Lending Club(−4)	0.004 (0.012)	−0.015 (0.032)
Lending Club(−3)	0.002 (0.012)	−0.024 (0.037)
Lending Club(−2)	−0.015 (0.012)	−0.011 (0.038)
Lending Club(−1)	Omitted baseline period	
Lending Club(0)	0.024 (0.013)*	0.027 (0.033)
Lending Club(+1)	0.023 (0.016)	0.067 (0.044)
Lending Club(+2)	0.021 (0.016)	0.070 (0.034)**
Lending Club(+3)	0.030 (0.014)**	0.046 (0.037)
Lending Club(+4)	0.054 (0.016)***	0.022 (0.035)
Lending Club(+5)	0.030 (0.018)*	0.046 (0.039)
Lending Club(+6)	0.024 (0.017)	0.063 (0.047)
Lending Club(+7)	0.047 (0.014)***	0.068 (0.038)*
Lending Club(+8)	0.061 (0.014)***	0.085 (0.033)**
Population	−0.003 (0.001)***	−0.003 (0.004)
Number of employed individuals	−0.008 (0.001)***	−0.004 (0.005)
Average monthly earnings	0.019 (0.006)***	−0.006 (0.021)
Median household income	−0.000 (0.001)	0.002 (0.002)
Labor force	0.004 (0.001)***	0.009 (0.008)
County fixed effects	✓	✓
Quarter fixed effects	✓	✓
n (counties)	2,309	339
n (observations)	64,590	9,492
R^2, including fixed effects	0.675	0.489

Note. Regressions are weighted using the CEM weights and standard errors (in parentheses) are clustered by county.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

maximum amount of Lending Club loans during our study period was relatively small ($35,000), we hypothesize that Lending Club has a larger impact on the financial health—and bankruptcy prospects—of nonbusinesses than of businesses (for which larger amounts are likely necessary to prevent—or cause—bankruptcy). We reran Specifications (1) and (2) with *nonbusiness bankruptcy filings per capita* and *business bankruptcy filings per establishment* as the dependent variables.[13] In both the all-state matched sample and the nine-state matched sample, we find parallel pretreatment trends for both nonbusiness and business bankruptcies (results available from authors). Table 6 shows that Lending Club approval has a significant effect on nonbusiness bankruptcy filings, but not on business bankruptcy filings. Our inability to detect a significant effect of Lending Club on business bankruptcy filings is consistent with our hypothesis and supports our causal interpretation.

Potential Concurrent (and Confounding) Events

Our analysis thus far indicates that bankruptcy filings increase after Lending Club approval. This could be due to the causal effect of Lending Club, but it could also be due to any other event or policy change that occurred in the treated states at the same time as Lending Club approval. We investigate this possibility both theoretically and empirically. Theoretically, we looked for state-level policy changes (particularly those related to bankruptcy exemptions and payday lending) that might have influenced bankruptcy filings during our time period. We could not find any major changes that coincided with Lending Club approval in the treated states, but not in the controls. Furthermore, we examined whether states granted Lending Club's license as part of a broader set of regulations/policies that might explain the rise in bankruptcy filings. We found no evidence for this. One indication that Lending Club approval was distinct from other policy changes is that its competitor Prosper.com (which has a similar business model) received regulatory approval in all states except for Iowa, Maine, and North Dakota in 2009.[14]

A feature of our setting that supports our causal interpretation is that Lending Club received approval at different times in the nine-state analysis: 2011-Q1 for counties in Kansas and North Carolina, 2013-Q1 for

Table 6. Regression Results for Bankruptcy Filings: Nonbusiness vs. Business Bankruptcy

Sample	All-state matched sample		Nine-state matched sample	
Dependent variable	Nonbusiness bankruptcy	Business bankruptcy	Nonbusiness bankruptcy	Business bankruptcy
Lending Club available	0.032 (0.008)***	0.065 (0.047)	0.048 (0.016)***	−0.059 (0.114)
Population	−0.003 (0.001)***	−0.007 (0.003)***	−0.003 (0.004)	0.002 (0.017)
Number of employed individuals	−0.008 (0.001)***	−0.014 (0.005)***	−0.004 (0.005)	−0.042 (0.028)
Average monthly earnings	0.020 (0.006)***	−0.000 (0.043)	−0.015 (0.021)	0.026 (0.135)
Median household income	−0.000 (0.001)	0.001 (0.005)	0.003 (0.002)*	−0.005 (0.018)
Labor force	0.004 (0.001)***	−0.005 (0.005)	0.009 (0.008)	0.029 (0.032)
County fixed effects	✓	✓	✓	✓
Quarter fixed effects	✓	✓	✓	✓
n (counties)	2,309	2,308	339	339
n (observations)	64,590	64,522	9,492	9,492
R^2, including fixed effects	0.681	0.077	0.494	0.059

Note. Regressions are weighted using the CEM weights and standard errors (in parentheses) are clustered by county.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

counties in Indiana, and 2013-Q2 for counties in Tennessee (see Table 1 and Online Appendix A). Thus, if an unobserved change is responsible for the effect that we attribute to Lending Club, then the change would have to have occurred at (or around) these specific times in the treated states, while not occurring in the control states. Although we believe this to be unlikely, we implemented two additional analyses to improve the evidence that the change in bankruptcy was caused by Lending Club approval rather than by an unobserved event: (1) a treatment-effect heterogeneity analysis and (2) a subsample analysis.

Treatment-Effect Heterogeneity Analysis. We exploited variation in the level of Internet access across counties to examine whether the effect that we attribute to Lending Club might have been caused by an unobserved factor. Because Lending Club is an online platform, the effect of its approval on bankruptcy filings should be larger in treated counties in which Internet access is widespread than in those in which Internet access is limited. We measured the level of Internet access for each county via its annual *Internet access scores* from the Federal Communications Commission's Form 477 County Data on Internet Access Services.[15] Scores are integers from zero to five and are based on the number of high-speed connections per 1,000 households in the county (e.g., zero represents zero connections, one represents between zero and 200 connections, etc.). We interacted *Lending Club available* with *Internet access score* for each county-quarter. The FCC measure is a yearly measure, so we used the yearly value for each quarter in a given year. Results are shown in Table 7. The *Lending Club available* coefficient is the baseline and represents the estimated treatment effect for county-quarter observations with *Internet access score* = 0. This effect is not distinguishable from zero. The coefficient for the interaction term represents the increase in the estimated treatment effect for each level of *Internet access score*. For example, the estimated treatment effect for county-quarter observations in the all-state matched sample with *Internet access score* = 1 is 0.024 (i.e., 0.016 + 0.008), which is significantly different from zero ($p = 0.012$). The estimated effect is larger at higher levels of Internet access. This provides additional evidence that the effect we observe is driven by Lending Club, and not by some unobserved confounding factor, because an unobserved confounding factor would be more likely to affect all counties equally.

Subsample Analysis. For the nine-state analysis, we reran the matching procedure and DiD analysis twice to correspond to the two periods in which Lending Club was approved in the treated states: 2011-Q1 for Kansas and North Carolina and 2013-Q1/Q2 for Indiana and Tennessee. The first analysis (referred to as the 2011 treatment analysis) tests the effect of Lending Club approval in Kansas and North Carolina counties in 2011, using Idaho, Iowa, Maine, Nebraska, and North Dakota counties as controls. The second analysis (referred to as the 2013 treatment analysis) tests the effect of Lending Club approval in Indiana and Tennessee counties in 2013, using Idaho, Iowa, Maine, Nebraska, and North Dakota counties as controls. We created separate matched samples for both analyses by matching treated and control counties on the values of *bankruptcy filings per capita* in the three (five) years preceding treatment for the 2011 (2013) analysis and on *population*. This yielded 272 matched counties for the 2011 analysis and 139 matched counties for the 2013 analysis.

Results of both analyses are shown in Table 8 and show a positive and significant effect of Lending Club approval. We confirmed that the pretreatment trends are parallel in the leads/lags model (results available from authors). Thus, if an unobserved change—concurrent with Lending Club approval—is responsible

Table 7. Regression Results for Bankruptcy Filings Per Capita: Treatment Effect Heterogeneity Based on Internet Access

Sample	All-state matched sample	Nine-state matched sample
Dependent variable	Bankruptcy filings per capita	
Lending Club available	0.016 (0.011)	0.027 (0.022)
Lending Club available × Internet access score	0.008 (0.003)***	0.011 (0.006)*
Internet access score	−0.003 (0.003)	−0.021 (0.007)***
Population	−0.004 (0.001)***	−0.004 (0.004)
Number of employed individuals	−0.008 (0.001)***	−0.003 (0.005)
Average monthly earnings	0.019 (0.006)***	−0.008 (0.022)
Median household income	−0.001 (0.001)	0.002 (0.002)
Labor force	0.004 (0.001)***	0.009 (0.008)
County fixed effects	✓	✓
Quarter fixed effects	✓	✓
n (counties)	2,309	339
n (observations)	64,590	9,492
R^2, including fixed effects	0.675	0.488

Note. Regressions are weighted using the CEM weights and standard errors (in parentheses) are clustered by county.
$^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

for the increase in bankruptcy filings, then this change would have to have occurred in 2011-Q1 in Kansas and North Carolina (and not the control states) and in 2013-Q1 in Indiana and in 2013-Q2 in Tennessee (and not the control states). This would be an unlikely coincidence, lending further support to our causal interpretation.

Effect of Similar Online Lending Platforms (e.g., Prosper.com)

To assess the robustness of our results, we examined the effect on bankruptcy filings of Prosper.com, which is an online lending platform similar to Lending Club. Like Lending Club, Prosper.com had to cease originating peer-to-peer loans temporarily to seek regulatory approval, after which they resumed origination. Unlike Lending Club, Prosper.com received regulatory approval from all states except Iowa, Maine, and North Dakota in 2009-Q3. Thus, there is little variation in Prosper.com's availability across counties and states when considering all 50 states. As a result, we conducted our analysis of Prosper.com using the nine-state sample.

First, we assessed whether the effect that we attribute to the approval of Lending Club might actually reflect the approval of Prosper.com, given the similarity in when the two received regulatory approval. We reran Specification (1) on the nine-state matched sample after adding a *Prosper.com available* dummy variable as a control. This sample includes counties in six states (Kansas, North Carolina, Indiana, Tennessee, Nebraska, and Idaho) that approved Prosper.com

Table 8. Regression Results for Bankruptcy Filings Per Capita: 2011 Treatment for Kansas and North Carolina and 2013 Treatment for Indiana and Tennessee

Sample	2011 Treatment (Kansas and North Carolina: 2008–2014)	2013 Treatment (Indiana and Tennessee: 2008–2014)
Dependent variable	Bankruptcy filings per capita	
Lending Club available	0.059 (0.017)***	0.058 (0.034)*
Population	−0.010 (0.003)***	0.005 (0.007)
Number of employed individuals	−0.003 (0.005)	0.009 (0.005)*
Average monthly earnings	0.006 (0.021)	−0.022 (0.054)
Median household income	0.002 (0.002)	−0.007 (0.004)
Labor force	0.005 (0.005)	−0.012 (0.011)
County fixed effects	✓	✓
Quarter fixed effects	✓	✓
n (counties)	272	139
n (observations)	7,616	3,892
R^2, including fixed effects	0.370	0.618

Notes. Regressions are weighted using the CEM weights. Standard errors (in parentheses) are clustered by county.
$^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

during the 2008 to 2014 analysis period (all in 2009-Q3) and counties in three states that did not (Iowa, Maine, and North Dakota). This treatment pattern creates a significant, but imperfect, correlation ($\rho = 0.58$) between *Prosper.com available* and *Lending Club available*, given that Kansas, North Carolina, Indiana, and Tennessee approved Lending Club later than Prosper.com and that Nebraska and Idaho did not approve Lending Club during the analysis period (see Table 1). As shown in column (1) of Table 9, the *Lending Club available* coefficient remains positive and significant after including *Prosper.com available*, and it is similar (0.045 versus 0.050) to the *Lending Club available* coefficient shown in Table 4, which is estimated without *Prosper.com available*. Because Prosper.com received approval before Lending Club in Kansas, North Carolina, Indiana, and Tennessee, the *Lending Club available* coefficient in column (1) can be interpreted as the additional impact of Lending Club approval in those states.

The insignificance of *Prosper.com available* in column (1) is likely because of its correlation with *Lending Club available* and because it does not vary as much as *Lending Club available*, not necessarily because the approval of Prosper.com had no discernible effect on bankruptcy filings. We explored this further by restricting our analysis to counties in the five states in which Lending Club had not been approved by the end of 2014 (see Table 1). This allowed us to isolate the effect of Prosper.com approval without potential contamination by Lending Club approval. Prosper.com was approved in two of these states in 2009-Q3 (Idaho and Nebraska), but not in the other three (Iowa, Maine, and North Dakota) by the end of 2014. Using data from 2008 to 2014, we reran Specification (1) with *Prosper.com available* in place of *Lending Club available* (which does not vary in this analysis). We used all counties from these states (the full sample) as well as a matched sample, using the matching process from above. Results are shown in columns (2) and (3) of Table 9 and show that Prosper.com approval had a positive effect on bankruptcy filings in this analysis (which is constructed to isolate the effect of Prosper.com). We also reran the model whose results are shown in column (1) after removing *Lending Club available*. This causes the *Prosper.com available* coefficient to become significant: $\beta = 0.039$, standard error = 0.020, $p = 0.050$. Overall, these analyses indicate that both online lending platforms impact bankruptcy filings.

Instrumental Variables Analysis

We further extended our analysis by using microlevel loan data from Lending Club to examine the relationship between the number of Lending Club loans and the number of bankruptcies. We used instrumental variables to improve the causal interpretation of our results. This complements the DiD analysis—which examines how approval of Lending Club relates to bankruptcies—by examining how the degree of Lending Club activity after approval relates to bankruptcies. This helps us assess the economic magnitude of the relationship.

Data, Variables, and Model Specification. Prior to 2021, Lending Club published loan data via its website.[16] We downloaded the data from 2007 to 2015 (n = 877,440 loans). This expands the time period used for the difference-in-differences analysis by one year pre and post and allows us to include Lending Club loans from when the platform first launched. The data describe each borrower (e.g., state of residence, self-reported income and debt-to-income ratio, and FICO credit score) and loan (e.g., grade assigned by Lending Club, origination date, size (i.e., amount), principal

Table 9. Regression Results for Bankruptcy Filings Per Capita: Effect of Prosper.com

Sample	Nine-state analysis	Restricted to five states (IA, ID, ME, ND, and NE)	
Matched or full sample	Matched sample	Full sample	Matched sample
Dependent variable		Bankruptcy filings per capita	
Lending Club available	0.045 (0.020)**	Not applicable	Not applicable
Prosper.com available	0.019 (0.023)	0.092 (0.020)***	0.080 (0.027)***
Population	−0.004 (0.004)	−0.006 (0.002)**	0.004 (0.007)
Number of employed individuals	−0.004 (0.005)	−0.006 (0.005)	−0.019 (0.008)**
Average monthly earnings	−0.010 (0.022)	0.015 (0.022)	0.032 (0.024)
Median household income	0.002 (0.002)	0.001 (0.002)	0.003 (0.002)
Labor force	0.011 (0.008)	0.006 (0.006)	0.011 (0.009)
County fixed effects	✓	✓	✓
Quarter fixed effects	✓	✓	✓
n (counties)	339	305	184
n (observations)	9,492	8,540	5,152
R^2, including fixed effects	0.488	0.410	0.467

Note. Regressions are weighted using the CEM weights and standard errors (in parentheses) are clustered by county.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

amount paid, term (36- or 60-month), purpose, and last payment date). We coded each loan as "outstanding" or "default" in each quarter as follows. We coded mature loans (i.e., those whose terms had expired) that Lending Club marked as paid as outstanding (i.e., current) in each quarter from loan origination to payoff. We coded immature loans listed as current or late as outstanding in each quarter from loan origination to the fourth quarter of 2015 (when our data collection stopped). We coded loans that Lending Club marked as in default or charged off as outstanding in each quarter from loan origination to the last payment quarter and then as default in subsequent quarters. We counted the number of loans outstanding in state i in quarter t. We conducted this analysis at the state-quarter level because Lending Club does not publish borrowers' counties. Table 10 reports descriptive statistics for the instrumental variables analysis.

Our specification is shown below:

$$Y_{it} = \alpha + \beta LoansOutstandingPerCapita_{it} + T_t + S_i + \gamma X_{it} + \varepsilon_{it}. \quad (3)$$

Y_{it} is *bankruptcy filings per capita* in state i in quarter t. The key independent variable is Lending Club *loans outstanding per capita* in state i in quarter t. T_t are quarter fixed effects, S_i are state fixed effects, X_{it} are control variables, and ε_{it} is the error term, which is clustered at the state level. β is the coefficient of interest. Ordinary least squares (OLS) estimation will yield a biased estimate of β due to endogeneity issues. Potential sources of endogeneity include: (a) omitted variables that affect both the number of loans and bankruptcy filings, such as state-specific economic health factors not otherwise accounted for by the state fixed effects or control variables; and (b) simultaneity (or reverse causality), by which bankruptcy filings influence the number of loans. To address this, we used instrumental variables and two-stage least squares (2SLS).

Instrumental Variable. Our instrumental variable, *debt-to-income policy change*, is based on a policy change made by Lending Club in the second quarter of 2012. Lending Club does not issue loans if an applicant's debt-to-income ("DTI") ratio exceeds a threshold. In the second quarter of 2012, Lending Club raised this threshold from 30 to 35 (technically from 0.3 to 0.35). This change should be associated with an increase in *loans outstanding per capita* because it expands the pool of qualified borrowers. We constructed the *debt-to-income policy change* instrument as the product of *Post DTI policy change* and *Time since 2012-Q1 (squared)*. *Post DTI Policy Change* is a dummy variable equal to one for observations after Lending Club's DTI policy change (i.e., after the first quarter of 2012) in states in which Lending Club was available to borrowers. It is equal to zero otherwise. *Time since 2012-Q1 (squared)* is the square of the number of quarters between quarter t and the first quarter of 2012. This causes the instrument to increase nonlinearly each quarter after the policy has been in place. This reflects the nonlinear accumulated growth in the pool of qualified borrowers in states in which Lending Club was available, which should correlate with growth in loans. We also used a linear version of this variable, which yields similar

Table 10. Descriptive Statistics of the State-Quarter Panel Used for the Instrumental Variables Analysis

Variable	Units	Min	Max	Mean	Median	Std. Dev.
Bankruptcy variables						
Bankruptcy filings per capita[a]	Continuous	0.11	3.10	0.87	0.79	0.43
Online lending variables						
Loans outstanding per capita	Continuous	0	4.05	0.41	0.06	0.69
Debt-to-income policy change	Continuous	0	255	31.47	0	59.96
Lending Club availability and maturity	Continuous	0	784	191.27	64	241.26
Median DTI of outstanding loans	Continuous	5.36	21.7	15.75	16.10	3.34
Demographic variables						
Population	Million	0.53	39.14	6.11	4.37	6.85
% age 60 & above	Percentile	10.8	24.9	18.7	18.8	2.3
% white	Percentile	24.9	96.2	77.3	79.3	13.6
% below high school attainment	Percentile	7.2	22	13.1	12.4	3.5
% housing units with mortgage	Percentile	31.8	54.3	44.3	44.6	4.7
Socioeconomic variables						
Number of employed individuals	Million	0.27	17.84	2.81	1.87	3.08
Average monthly earnings	Thousand dollars	2.64	7.50	3.85	3.69	0.72
Median household income	Thousand dollars	35.63	74.55	52.52	50.45	8.65
Labor force	Million	0.37	24.90	3.95	2.64	4.39

Note. Std. Dev., standard deviation.
[a]Because the Federal Judicial Center data only includes bankruptcy data starting in 2008, we used bankruptcy data from the Administrative Office of the U.S. Courts website for the year 2007 observations.

results. The *debt-to-income policy change* instrument leverages different variation (Lending Club's policy change) than does the difference-in-differences analysis (state regulator's approval of Lending Club), which increases the robustness of our analysis. Online Appendix G reports the first-stage results of the 2SLS regression. The instrument is relevant and strong, as shown in Table 11.

We examined the exogeneity of *debt-to-income policy change* as follows. As mentioned above, if state-specific economic health factors (not otherwise accounted for by our control variables or by the state fixed effects) affect both the number of loans and bankruptcy filings, then the effect of these factors would be absorbed in the error term. Thus, a necessary condition for *debt-to-income policy change* to be exogenous is that it be uncorrelated with state-specific economic health. This condition holds, because the DTI policy change was a blanket change applied across all states, as opposed to being applied mostly to states with poor (or good) economic health. Thus, there should be no correlation between *debt-to-income policy change* and state-specific economic health. Another potential concern is that *debt-to-income policy change* is likely to affect bankruptcy filings through not only the number of Lending Club loans (i.e., *loans outstanding per capita*), but also through the quality of Lending Club loans, which might decline after the policy change because borrowers with higher DTI ratios would be able to receive loans. We accounted for this by including *median DTI of outstanding loans* to control for loan quality; this variable is the median DTI of outstanding Lending Club loans in state i in quarter t.[17] To further examine the exogeneity of *debt-to-income policy change*, we identified an additional instrumental variable: *Lending Club availability and maturity*. This gave us more instruments than endogenous variables, allowing us to assess instrument exogeneity via a test of overidentifying restrictions. *Lending Club availability and maturity* is the product of *Lending Club available* and *Time since 2008-Q4 (squared)*. *Lending Club available* reflects whether Lending Club was available to borrowers in state i in quarter t; it is the same dummy variable used in the DiD analysis, although measured at the state (instead of county) level. *Time since 2008-Q4 (squared)* is the square of the number of quarters between quarter t and the fourth quarter of 2008. This captures how Lending Club matured over time after it received regulatory approval from most states in 2008-Q4 (see Table 1). *Lending Club availability and maturity* is relevant and strong, as shown in the second row of Table 11. It should also be exogenous given that Lending Club's availability in each state is more likely determined by the (quasi-random) timing of its approval by regulators than by unobserved factors that also affect bankruptcy filings (as explored extensively in the DiD analysis).

Results of Instrumental Variables Analysis. Table 12 shows the instrumental variables results, along with the ordinary least squares results. The OLS coefficient for *loans outstanding per capita*, shown in column (1), is insignificant and negative. The negative coefficient may be because of a simultaneity/reverse causality issue: namely, that a high level of bankruptcy filings is likely to reduce the number of Lending Club loans because bankrupt individuals will not qualify for loans. This highlights the need to use an instrumental variables approach. Columns (2) and (3) show the 2SLS results using *debt-to-income policy change* by itself and both instruments together. The 2SLS coefficients for *loans outstanding per capita* coefficients are positive, significant, and similar in magnitude across models. When using both instruments, we are unable to reject (via a test of overidentifying restrictions) the null hypothesis that the instruments are exogenous (Hansen's J statistic = 1.77, p = 0.18). The 2SLS coefficient when using both instruments is 0.034. Using this coefficient, a one-standard-deviation increase in *loans outstanding per capita* (δ = 0.69) is associated with a 0.023 increase in *bankruptcy filings per capita*. Because the mean of *bankruptcy filings per capita* is 0.87, this represents a 2.7% increase. The coefficient also implies that an increase of 100 loans per capita in a quarter is associated with an increase of 3.4 bankruptcies per capita. This is a fairly large effect size, which we investigate below.

We conducted several additional analyses and robustness checks. First, we reran the regressions using lagged values of *loans outstanding per capita* and the

Table 11. Instrument Relevance/Strength Statistics

Instrument	First-stage adjusted R^2	Partial R^2 (attributable to instrument)	Underidentification test	Weak identification test
Debt-to-income policy change	0.94	0.47	6.42 (p = 0.011)	343.75 (16.38) $p < 0.001$
Lending Club availability and maturity	0.92	0.35	6.76 (p = 0.009)	59.02 (16.38) $p < 0.001$

Note. The Kleibergen–Paap Lagrange multiplier statistic is reported in the underidentification test; the Kleibergen–Paap Wald F statistic is reported in the weak identification test with Stock–Yogo 10% instrumental variable size thresholds reported in parentheses.

Table 12. Regression Results for Bankruptcy Filings Per Capita: Instrumental Variables Results

Dependent variable	Bankruptcy filings per capita		
Model	OLS	IV: Debt-to-income policy change	IV: Both instruments
Loans outstanding per capita	−0.017 (0.032)	0.037 (0.016)**	0.034 (0.016)**
Median DTI	0.003 (0.001)*	0.002 (0.001)	0.002 (0.001)
Population	0.241 (0.282)	0.248 (0.291)	0.248 (0.290)
Number of employed individuals	−0.475 (0.089)***	−0.482 (0.092)***	−0.482 (0.092)***
Average monthly earnings	−0.071 (0.049)	−0.068 (0.046)	−0.068 (0.046)
Median household income	0.014 (0.013)	0.012 (0.013)	0.012 (0.013)
Labor force	−0.083 (0.447)	−0.106 (0.462)	−0.105 (0.461)
% age 60 & above	−0.006 (0.033)	−0.007 (0.033)	−0.007 (0.033)
% white	0.033 (0.017)*	0.034 (0.018)*	0.034 (0.018)*
% below high school attainment	0.043 (0.032)	0.040 (0.031)	0.040 (0.031)
% housing units with mortgage	0.056 (0.029)*	0.062 (0.033)*	0.062 (0.032)*
State fixed effects	✓	✓	✓
Quarter fixed effects	✓	✓	✓
n (states)	51	51	51
n (observations)	1,835	1,835	1,835
R^2	0.91	0.92	0.92
Hansen's *J* statistic	Not applicable	Not applicable	1.77 (p = 0.18)

Notes. Standard errors (in parentheses) are clustered by state. IV, instrumental variables.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

instruments. For lags, we used the previous quarter, the previous two quarters, the previous three quarters, and the previous four quarters. This reflects the possibility that the effect of Lending Club loans on bankruptcy filings does not show up immediately. These results are consistent, with the magnitude of the coefficients increasing slightly with lag length. Second, we confirmed that the results are robust after we removed potentially fraudulent loans that Lending Club allegedly loaned to itself to boost its loan-volume statistics.[18] Third, we confirmed that the results are robust when using alternative independent variables, including the natural log of loans per capita and the value of loans (instead of the number of loans).

Mechanisms for the Lending Club Effect

As described in the Background, Literature Review, and Motivation sections, there are several mechanisms that could explain the link between Lending Club approval and increased bankruptcy filings. One potential mechanism, referred to as the "credit-risk" mechanism, is that Lending Club borrowers are high credit risks, who do not have access to traditional capital. If so, then Lending Club loans could be going to people who are inherently uncreditworthy, thereby leading them into bankruptcy. A second potential mechanism is that Lending Club borrowers are normal credit risks, who have access to traditional capital. However, the ease of getting a Lending Club loan may cause these borrowers to become overextended financially, leading to bankruptcy. We refer to this as the debt-trap mechanism. A third potential mechanism is that Lending Club provides an incentive for borrowers to file for bankruptcy when they might not otherwise. For example, strategic borrowers might use Lending Club loans to swap secured debt (e.g., a car loan) for unsecured debt (e.g., a Lending Club loan). This debt restructuring might increase the perceived benefit of declaring bankruptcy, thereby nudging marginal borrowers to file. We examined these three mechanisms empirically.

We examined the credit-risk mechanism by reviewing the FICO scores of Lending Club borrowers from the microlevel loan data. The mean FICO score range was 695 to 699 (the median range was 690 to 694), which is similar to the mean FICO scores reported in credit-bureau data (Jagtiani and Lemieux 2019). Jagtiani and Lemieux (2019) also find that based on observable credit features, borrowers get lower interest rates from Lending Club than from traditional lenders. Based on this, it does not appear that Lending Club attracts and issues loans to borrowers who are systematic credit risks. However, because FICO score is only one measure of creditworthiness, we examined the credit-risk mechanism, along with the debt-trap mechanism, further by leveraging borrowers' assets and liabilities. The Federal Judicial Center bankruptcy data report assets and liabilities of bankruptcy filers. Assets and liabilities are reported only as a range, including zero to 50,000; 50,000 to 100,000; and 100,000 to 500,000 (higher ranges are also possible). We divided the bankruptcy filing data used in the DiD analysis into four subgroups: (1) filers with assets between zero and 50,000, (2) filers with assets between 50,000 and 500,000, (3) filers with liabilities between zero and 100,000, and (4) filers with liabilities between 100,000 and 500,000. (We excluded filings with more than

500,000 in assets or liabilities, because those are likely business bankruptcies.) We then computed *bankruptcy filings per capita* per county-quarter for each subgroup. We used different breakpoints for the subgroups defined by assets and liabilities (50,000 versus 100,000) so that the mean of *bankruptcy filings per capita* was similar in all subgroups, to help with interpretation of the effect sizes.

On one hand, if the credit-risk mechanism drives our results, then Lending Club approval should increase bankruptcy filings for only the borrower subgroups with low levels of assets and liabilities, because borrowers in these subgroups are likely to have low incomes and little access to capital. By the same logic, Lending Club approval should *not* increase bankruptcy (or at least not as much) for borrower subgroups with higher levels of assets and liabilities. On the other hand, if the debt-trap mechanism drives our results, then Lending Club approval should increase bankruptcy filings for borrowers in *all* subgroups, because any borrower—regardless of assets and liabilities—could become overextended after getting a Lending Club loan.

We reran Specification (1) for each subgroup using the all-state matched sample. Columns (1) and (2) in Table 13 show that Lending Club increases bankruptcy filings in both subgroups defined by assets, although the magnitude (both in absolute terms and as a percentage of the mean, which is reported in the last row of Table 13) is larger for borrowers with higher levels of assets. Columns (3) and (4) in Table 13 show a similar pattern, with a larger magnitude for borrowers with higher levels of liabilities. This suggests that it is more likely that Lending Club borrowers become overextended and fall into a debt trap, as opposed to being inherent credit risks. The self-reported income of Lending Club borrowers (from the Lending Club loan data) provides further support for the debt-trap mechanism. As shown in Online Appendix H, Lending Club borrowers, including those who default on their loans, report substantially higher-than-average incomes.

We further examined the debt-trap mechanism by using the microlevel loan data to analyze what factors predict whether a Lending Club borrower will default on his loan (coded as *Loan default* = 1). In our first analysis, we included only mature loans. This ensured that if a borrower defaulted during the term of the loan that we would observe it. We observe individual loan defaults, but we cannot connect them to individual bankruptcies. However, the two should be related, because borrowers who do not repay their loans are more likely to file for bankruptcy than borrowers who repay. Thus, we assume that factors that lead to loan default also lead to bankruptcy. The key independent variable in this analysis is *debt expansion*, which is a

Table 13. Regression Results for Bankruptcy Filings Per Capita: Subgroup Analysis Based on Borrower Assets and Liabilities

Sample	All-state matched sample			
Dependent variable	Bankruptcy per capita with assets [0, 50,000]	Bankruptcy per capita with assets (50,000, 500,000]	Bankruptcy per capita with liabilities [0, 100,000]	Bankruptcy per capita with liabilities (100,000 500,000]
Lending Club available	0.015 (0.005)***	0.022 (0.005)***	0.008 (0.005)*	0.023 (0.005)***
Population	−0.000 (0.000)	−0.002 (0.001)**	−0.000 (0.000)	−0.001 (0.001)**
Number of employed individuals	−0.001 (0.001)**	−0.007 (0.001)***	0.000 (0.000)	−0.006 (0.001)***
Average monthly earnings	−0.001 (0.004)	0.0019 (0.004)***	−0.001 (0.004)	0.016 (0.004)***
Median household income	−0.001 (0.000)**	0.000 (0.001)	−0.001 (0.000)*	−0.000 (0.000)
Labor force	0.001 (0.001)**	0.003 (0.001)***	0.001 (0.001)**	0.002 (0.001)***
County fixed effects	✓	✓	✓	✓
Quarter fixed effects	✓	✓	✓	✓
n (counties)	2,309	2,309	2,309	2,309
n (observations)	64,590	64,590	64,590	64,590
R^2, including fixed effects	0.532	0.592	0.564	0.584
Mean of dependent variable	0.303	0.336	0.312	0.307

Note. Regressions are weighted using the CEM weights and standard errors (in parentheses) are clustered by county.
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

dummy variable that indicates whether the purpose of the loan was to expand the borrower's overall debt or to consolidate it (e.g., by using a Lending Club loan with a 10% interest rate to pay off credit card debt with a 22% interest rate). For each loan application, the borrower selects the purpose of the loan from a predefined list. Two choices indicate debt consolidation: "credit card" and "debt consolidation." We coded *debt expansion* = 1 for loans with a purpose *other than* these two; examples include home improvement, major purchase, and vacation. We included several other variables published by Lending Club as controls. We used a linear probability model to predict *loan default*, although results from a logistic regression are similar. The results are shown in column (1) of Table 14. The coefficient for *debt expansion* ($\beta = 0.014$) is positive and significant. Because 13.2% of loans in the sample ultimately default, the *debt expansion* coefficient represents a 10.6% increase in the probability of default. In our second analysis, we used a Cox proportional hazards model to assess what factors, including loan purpose, affect the "hazard" of a loan defaulting. For this analysis, we used immature loans, given that hazard models accommodate right censoring. Column (2) of Table 14 shows that *debt expansion* loans are more likely to default. These results indicate that Lending Club loans that increase a borrower's overall debt are positively associated with default (and potentially bankruptcy). Given that 18% of loans appear to fall into this category (the mean of *debt expansion* is 0.18), this provides additional support for the debt-trap mechanism.

We examined the strategic-borrowing mechanism by analyzing when borrowers who default stopped repaying their loans. If borrowers use a Lending Club loan to pay off another (likely secured) loan with no intention of repaying the (unsecured) Lending Club loan, then they should stop repaying the Lending Club loan relatively quickly. For each defaulted loan, we computed the number of months between loan origination and the last payment date. Figure 1 shows that for approximately 18% (6.5%) of loans that default, the borrowers ceased repayment after six (three) months. This suggests that strategic borrowing may explain some of the effect of Lending Club on bankruptcy filings. The finding that Lending Club has a larger effect for borrowers with more liabilities (see Table 13, columns (3) and (4)) also provides support for the strategic-borrowing mechanism. This is because the strategic-borrowing mechanism assumes that a borrower has a debt liability (such as a car loan) to restructure.

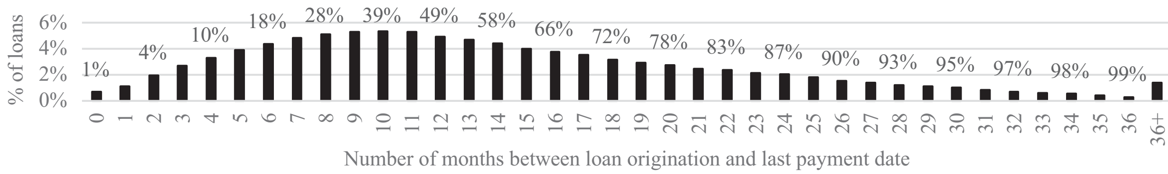
To deepen our analysis of the mechanisms, we considered how much of the effect size implied by the instrumental variables analysis—that an increase of 100 loans per capita in a quarter is associated with an increase of 3.4 bankruptcies per capita—can be explained by the direct mechanisms discussed above. If every loan that defaulted because of the debt-trap or strategic-borrower mechanisms resulted in a bankruptcy, then the default rate would need to be 3.4% per quarter to account for the effect size. The default rate of loans published in the microdata is 1.1% per quarter. One potential explanation for this gap is that the published Lending Club loan data systematically undercounts the number of Lending Club loans. This occurs because Lending Club also operates loan programs outside of its standard marketplace (e.g., see Lending Club's 2015 10-K). We compared the total amount loaned from 2007 to 2015 as reported in Lending Club's 2015 10-K (https://www.sec.gov/Archives/edgar/data/1409970/000140997016001762/a201510-k.htm) to the total amount of loans published in the microdata. The total amount in the microdata

Table 14. Regression Results for Loan Default: Loan-Level Analysis

Dependent variable	Loan default	
Model	Linear probability	Hazard
Debt expansion	0.014 (0.002)***	1.169 (0.012)***
Debt to income ratio (preloan)	0.002 (0.000)***	1.001 (0.000)***
Loan amount	0.0003 (0.0001)***	1.012 (0.001)***
Annual borrower income	−0.0002 (0.0000)***	0.996 (0.001)***
36 month loan term (= 1 if 36-month term and = 0 if 60-month term)	−0.064 (0.004)***	1.364 (0.013)***
FICO score (low end of range)	−0.0002 (0.0000)***	0.988 (0.000)***
Loan grade dummy variables (A1-G5, assigned by Lending Club)	✓	
Borrower's state of residence dummy variables	✓	
Month-year of loan origination dummy variables	✓	
n (observations)	209,882	677,554
R^2, including fixed effects	0.039	Not applicable

Note. Standard errors (in parentheses) are clustered by month of loan origination.
$^*p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Figure 1. Histogram of How Many Months Borrowers Repay Loans that End in Default



Note. The numbers listed at the top of the data columns are cumulative percentages.

accounts for approximately 81% of the total amount in the 10-K; thus, our measure of *loans outstanding per capita* is undercounted. If the nonpublished loans yield the same outcomes (on average) as the published loans, then our coefficient (and effect size) will be inflated. To examine this, we reran Specification (3) after multiplying *loans outstanding per capita* by 1.23 (i.e., 100/81). This narrows the gap by reducing the *loans outstanding per capita* coefficient from 0.034 to 0.028 ($p < 0.10$).[19]

Despite this, the direct mechanisms examined above are unlikely to explain the full effect size. This suggests that part of the Lending Club effect is driven by indirect mechanisms. One possibility is that traditional lenders respond to competition from Lending Club by issuing riskier loans (in order to maintain consistent loan volumes) that result in borrowers' bankruptcy (Cornaggia et al. 2019). Similar risky behavior—and increased bankruptcies—occurred when competition among banks increased after deregulation (Dick and Lehnert 2010; see the Literature Review section). We explored this by examining whether the effect of Lending Club depends on the likely level of competition among traditional lenders. If competition among traditional lenders in a county is high, then new competition from Lending Club should have little effect on traditional lenders' behavior, because Lending Club is just one more competitor among many. On the other hand, if competition among traditional lenders in a county is low, then new competition from Lending Club is more likely to affect their lending practices, such as prompting them to issue riskier loans to maintain loan volumes. We measured competition among traditional lenders as *bank branches per capita*, which is the number of bank branches in county i at quarter t per 1,000 people. We used the Branch Office Deposits data published by the Federal Deposit Insurance Corporation to construct this variable.[20] Higher levels of *bank branches per capita* indicate greater competition. We added *bank branches per capita* and its interaction with *Lending Club available* to Specification (1). As shown in column (1) of Table 15, the baseline effect of *Lending Club available* is positive and significant, and the interaction coefficient is negative and significant. This is consistent with the proposed mechanism: The effect of Lending Club is larger in counties where existing competition is likely to be low, such that new competition from Lending Club yields a stronger competitive response. However, this mechanism assumes that there are traditional lenders to respond to competition from Lending Club, which may not be true for counties with zero bank branches. To examine this, we reran the regression after removing observations for which the value of *bank branches per capita* was zero. Results appear in column (2) of Table 15 and are consistent with those in column (1).

Discussion and Implications

Using different identification strategies, data samples, and levels of analysis, we consistently find a positive relationship between online lending and bankruptcy filings. The difference-in-differences leads/lags analysis (reported in Table 5) indicates that Lending Club approval has both short-term and long-term effects. The short-term effect may be due (in part) to strategic borrowing, in which borrowers use a Lending Club loan to restructure their debt so that bankruptcy becomes more attractive, thereby nudging them to file when they might not have otherwise. The long-term effect is likely due (in part) to both the strategic-borrowing and debt-trap mechanisms, given that it takes time for the debt-trap mechanism to operate: Borrowers must receive a Lending Club loan, have the additional debt burden drive them to bankruptcy, and then file bankruptcy. Indirect mechanisms, such as the responses of traditional banks to competition from Lending Club, may also play a role.

The strategic-borrowing mechanism represents adverse selection. This is because there is information asymmetry between strategic borrowers and investors before the loan is issued: The borrowers know that they have no intention to repay the Lending Club loan, but withhold this information from investors. There is also a possibility of moral hazard, in the sense that borrowers may decide after they receive the loan that there are minimal consequences if they choose to declare bankruptcy rather than to repay the loan. For example, Du et al. (2020) study how behavioral mechanisms can mitigate moral hazard in a Chinese online lending platform. However, we find moral hazard unlikely in the U.S. context, given the consequences of

Table 15. Regression Results for Bankruptcy Filings Per Capita: Competitive Response Mechanism

Dependent variable	Bankruptcy filings per capita	
Model	All-state matched sample	Excluding counties with zero bank branches
Lending Club available	0.038 (0.008)***	0.045 (0.009)***
Lending Club available × Bank branches per capita	−0.009 (0.004)**	−0.009 (0.004)**
Bank branches per capita	0.010 (0.006)[a]	0.013 (0.006)**
Population	−0.003 (0.001)***	−0.002 (0.001)**
Number of employed individuals	−0.008 (0.001)***	−0.007 (0.001)***
Average monthly earnings	0.019 (0.006)***	0.026 (0.006)***
Median household income	−0.001 (0.001)	−0.001 (0.001)
Labor force	0.004 (0.001)***	0.003 (0.001)**
County fixed effects	✓	✓
Quarter fixed effects	✓	✓
n (counties)	2,309	1,813
n (observations)	64,590	48,829
R^2, including fixed effects	0.675	0.702

Note. Regressions are weighted using the CEM weights and standard errors (in parentheses) are clustered by county.
[a]$p = 0.130$; *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

declaring bankruptcy. Furthermore, we believe that most borrowers (other than strategic borrowers) are sincere in their intention to repay, but that some are unable to, perhaps due to unforeseen circumstances or because they misjudged their financial status.

The effect of online lending on bankruptcy filings can be mitigated by policy changes by online lending platforms; indeed, Lending Club has made several such changes. For example, in 2017, Lending Club removed the riskiest loans (grades F and G) from its platform, citing their high delinquency rates, and it launched a hardship program to help struggling borrowers. Also, Lending Club introduced its Direct Pay program shortly after our study period and expanded this program in 2019 by creating balance transfer loans.[21] This program sends a portion of a loan directly to the borrower's creditor(s), which essentially requires that the borrower use the loan (or at least a portion thereof) for debt consolidation instead of debt expansion. Programs like this, particularly if implemented for borrowers most likely to default, could diminish or potentially eliminate the effect that we document. Studying the effect of these programs is a promising avenue for future research. Increased education about financial management, which could be provided by online lending platforms to borrowers during the loan-application process, may also help.

Conclusion

Online lending platforms have great potential to improve individuals' financial health and security by providing easy access to affordable credit. However, they could also lure borrowers into a debt trap that leads to bankruptcy. We exploit variation in when states granted approval for Lending Club to issue peer-to-peer loans to examine Lending Club's effect on bankruptcy filings. We conduct a difference-in-differences analysis with both the all-state matched sample and the nine-state matched sample. We also conduct an instrumental variables analysis to examine the relationship between the number of Lending Club loans and bankruptcy filings. We consistently find that Lending Club increases bankruptcy filings. We identify possible mechanisms for the effect, including: (1) the ease of receiving a Lending Club loan lures borrowers into a debt trap that results in bankruptcy; (2) strategic borrowers use Lending Club loans to restructure their debt to make bankruptcy more attractive; and (3) traditional lenders respond to competition from Lending Club loans by issuing riskier loans that result in borrowers' bankruptcy.

Our study contributes to both the online-platforms literature and the bankruptcy literature. Online platforms are growing quickly, and regulators and researchers are unsure of their impacts on society and the economy. Our findings point to a potential dark side of online lending platforms: increased bankruptcy. To be clear, we are not arguing that online lending platforms are "bad" for the economy or for society. They likely have positive effects that we do not explore. Instead, we identify a specific issue that could potentially be addressed via platform design or regulation. Indeed, recent initiatives by Lending Club may address the issue, at least in part.

Our study has limitations. First, we cannot be sure that our results apply to all online lending platforms, although Lending Club is the largest platform, and our results also appear to hold for Prosper.com. Second, our results may be specific to the time period of our sample, which is a common limitation of empirical studies. It is possible that recent online loans are less (or more) likely to contribute to bankruptcy. Third, because Lending Club does not identify individual borrowers, we cannot connect individual Lending Club borrowers to bankruptcy records. Instead, we use aggregate data (at the county and state levels) to identify the impact of Lending Club. This lack of individual-level data is common in studies such as ours that investigate the societal impacts of online platforms (Chan and Ghose 2014, Seamans and Zhu 2014, Greenwood and Agarwal 2016, Greenwood and Wattal 2017, Burtch et al. 2018). Future research can leverage individual-level data (if available) to provide additional insight into the mechanisms by which online lending platforms affect financial well-being.

Endnotes

[1] For example, see https://www.lendingclub.com/company/about-us.

[2] See https://www.treasury.gov/connect/blog/Pages/Opportunities-and-Challenges-in-Online-Marketplace-Lending.aspx.

[3] https://p2pmarketdata.com/p2p-lending-funding-volume-usa/.

[4] See https://www.lendingclub.com/loans/personal-loans/balance-transfer.

[5] Secured debts are secured by property, such that the creditor can claim the property securing the debt if the borrower defaults. For unsecured debts, there is no such collateral.

[6] See https://www.thebankruptcysite.org/resources/bankruptcy/chapter-13/secured-vs-unsecured-debt-chapter-13-bankruptcy and https://www.thebalance.com/filing-chapter-7-bankruptcy-527442 for details.

[7] See https://www.lendacademy.com/a-look-back-at-the-lending-club-and-prosper-quiet-periods/.

[8] See https://help.lendingclub.com/hc/en-us/articles/213706208-Qualifying-for-a-personal-loan.

[9] We exclude Mississippi because its bankruptcy trend differs from the other states in this analysis. In the nine focal states, bankruptcy filings per capita declined year-over-year from 2010 to 2014. This is not true for Mississippi, which experienced a pronounced increase in bankruptcy filings in 2013. This suggests a possible policy change or economic shock—specific to Mississippi—that could confound our estimation of the effect of Lending Club.

[10] Because federal courts have jurisdiction over personal and business bankruptcy cases, these data fully represent the bankruptcy activity of individuals and businesses in the United States. Bankruptcy filing data are available from https://www.fjc.gov/research/idb/bankruptcy-cases-filed-terminated-and-pending-fy-2008-present and http://www.uscourts.gov/statistics-reports/caseload-statistics-data-tables.

[11] We matched *bankruptcy filings per capita* in 2008-Q4, 2009-Q4, 2010-Q1, 2010-Q2, 2010-Q3, and 2010-Q4 and *number of employed individuals* in 2008-Q4, 2009-Q4, and 2010-Q4. Finer/stricter matching reduces the number of observations in the matched sample, but returns similar results.

[12] Assume that Lending Club is approved in Iowa sometime in the future. In that case, all observations in our panel for Iowa counties would be from time periods eight or more quarters before Lending Club approval in Iowa. To explore this possibility, we reran the regression after setting $LC_{it-8} = 1$ for all observations from Iowa counties. This has no substantive effect on our results; given our inclusion of county fixed effects, this affects only the α constant.

[13] We measured *Business bankruptcy filings per establishment* as the number of business bankruptcy filings × 1,000/number of establishments, which we collected from the County Business Patterns data from the Census.

[14] See p. 1 of https://www.prosper.com/Downloads/Legal/prosper10k123109%20(3.31.2010%20final).pdf.

[15] See https://www.fcc.gov/form-477-county-data-internet-access-services.

[16] Although Lending Club no longer publishes loan data via its website, it reports loan data to the Securities and Exchange Commission (see https://www.sec.gov/cgi-bin/browse-edgar?filenum=333-233190).

[17] *Median DTI of outstanding loans* is undefined for state-quarter observations with zero Lending Club loans. We replaced these missing values with the mean of the observed *Median DTI* values for each state. Note that our fixed-effects regression effectively demeans the explanatory variables. As a result, the de-meaned version of these missing values is zero, such that they do not affect the estimation. For states in which Lending Club was not approved during the analysis period, *Median DTI* is always zero.

[18] We define fraudulent loans as those that are fully paid within two billing cycles. The ratio of fraudulent loans to total loans is 246 to 887,440.

[19] A more formal way to see this relationship is via the simple regression coefficient formula: $\beta = \sum_1^n (x_i - \bar{x})(y_i - \bar{y}) / \sum_1^n (x_i - \bar{x})^2$. If x is really $2x$, such that $(x_i - \bar{x})$ is really $(2x_i - 2\bar{x})$, then β will be half as large.

[20] See https://www7.fdic.gov/sod/dynaDownload.asp.

[21] See https://debanked.com/2017/11/lending-club-is-discontinuing-issuance-of-f-and-g-grade-notes/, https://help.lendingclub.com/hc/en-us/articles/115004323368-Recent-and-upcoming-changes-to-the-downloadable-data-files-and-API-services, https://www.lendacademy.com/lendingclub-expands-program-to-help-borrowers-actually-pay-off-debt/, and https://www.lendingclub.com/loans/personal-loans/balance-transfer.

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